LETTERS TO THE EDITOR.

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Professor Boltzmann's Letter on the Kinetic Theory of Gases.

IN common, I am sure, with all the physical readers of NATURE, I have read Herr Boltzmann's letter with great interest. And I am glad to observe that, though he appears to think I differ from him, that part of his letter which chiefly deals with my criticism on Dr. Watson's idea of what "Boltzmann's Minimum Theorem" is, is simply putting forward, with all his great authority, the view for which I contended. But it is a little hard that Dr. Boltzmann should represent me as endeavouring to *disprove* his theorem when I expressly stated that while I did not know *his* proof, *I supposed that it was all right.* True, I said that I found it hard to conceive how any proof on the lines of Dr. Watson's could be valid *because that proof appeared to me to be a purely dynamical proof*, and I applied the reversibility argument *to show that a purely dynamical proof was impossible*, so that the H-theorem could not be a purely dynamical theorem; and after indicating the lines on which it appeared that there might be an *average dynamical* theorem, I asked if some one would say what the H-theorem really was.

Thereupon Mr. Burbury wrote a helpful letter, which he followed up by a still more helpful correspondence, in which verbal misunderstandings were gradually cleared away, which showed that the proof of the H-theorem considered as a dynamical theorem, not as a theorem in probabilities, assumed that in one respect the configuration was, before each set of collisions, already perfectly average, and that this condition is violated in the reversed motion; so that the theorem, regarded as a dynamical theorem, is not proved for configurations in general, but for those possessing a certain amount of "average" already—a restriction which comes to the same thing as the limitation imposed by Prof. Boltzmann when he says *the theorem is not a dynamical theorem, but one in probabilities.*

Shortly after Mr. Burbury's letter appeared, Dr. Watson wrote denying that the criticism from reversibility applied, and claiming that the theorem was a general dynamical theorem, in the sense that it applied to *all* configurations. Enlightened by Mr. Burbury, I now see that Dr. Watson's reasoning is not open to the objection that it proves a general dynamical theorem; but I cannot blame myself for thinking that it did, for that was what Dr. Watson himself believed it to do, and what his language naturally implies. Moreover, after perceiving the oversight which vitiates the proof in its present form, I did not examine it further.

Prof. Boltzmann has misunderstood Mr. Burbury and me in one or two particulars. He denies that there are as many configurations for which dH/dt is positive as there are for which it is negative. He evidently thinks that we mean something different from the bare meaning of the words, which are certainly true. It is easy to explain what we do not mean (I say *we*, for I am sure Mr. Burbury will agree with me). Suppose $H = 10$ to be the minimum value of H for a given system of molecules, we do not mean that among all the configurations for which $H = 50$, there are as many which will, if left to themselves, turn into configurations for which $H = 60$, as will turn into configurations for which $H = 40$. The illustration, which to my mind has most clearly removed the apparent contradiction in the statement that there are as many configurations for which H will increase as decrease, while yet the probability is that H will on the whole decrease, is that of a *y* turned upside down, thus ʎ. For every downward path there is an upward path, *i.e.* the reversed direction; yet starting from the angle there are two ways down for one way up, so that there is a greater probability of going down than up. If in the reversibility argument one could assert, not merely that there are *as many* configurations for which H tends to increase as to decrease, but that *for any given value of* H there were as many configurations which tend to increase as to decrease, then the conclusion that H was as likely to increase as to decrease could be deduced. But the argument is quite invalid when we set off a configuration for which H increases against one for which it decreases, *although the values of H for each are different.* As an illustration more closely allied to the case of a gas, we might take a tree turned upside down, with an infinite number of branches passing through each point of its substance in all directions, there being at every point more branches tending downward than upward (because those whose tangents are horizontal may be said to tend downward on each side), and every upward branch finally turns downward and tends to become nearly horizontal at last, when H is near its minimum value.

To my mind this appears a far better way of meeting the difficulty than Prof. Boltzmann's illustration of the dice, for so far as I can see, all that he has shown is that if you start from an exceptionally high ordinate, *i.e.* one over the average, you are likely, after a considerable time, to get to lower ordinates in whichever direction you go, and an opponent might answer that if you start from an exceptionally low ordinate you are likely to get higher ones in whichever direction you go, and that there must of course be as many deviations below the average as above it, so that if you start from an arbitrary point in an arbitrary direction, you are just as likely to get to higher as to lower ordinates. In point of fact this appears to be the case for his curve, while it is not true for the tree or for a gas.

Prof. Boltzmann must have put an entirely wrong construction on something or other, which I suppose I have written, when he says I object to the Maxwell Law of distribution because it would ultimately lead to the total kinetic energy of the universe being equally distributed among every degree of freedom of every particle in the universe. Instead of considering that to be *à priori* improbable, I hold exactly the view put forward by Prof. Boltzmann.

With regard to the first portion of Prof. Boltzmann's letter, there is so much that is speculative in it that any discussion would occupy more space than I feel entitled to claim. I will only say that the idea that a gas takes years to come to thermal equilibrium seems hardly consistent with vibrational portion of the kinetic theory being of practical value, when applied to gas which has only had a few hours to settle down.

EDWARD P. CULVERWELL.

Trinity College, Dublin, March 6.

IT seems to me that my meaning has not been expressed quite clearly; therefore, it may be worth while to add one remark. Not for every curve, but only for the particular form of the H-curve, disymmetrical in the upward and downward direction, can it be proved that H has a tendency to decrease. This particular form is very well illustrated by Mr. Culverwell's suggestion of an inverted tree. The H-curve is composed of a succession of such trees. Almost all these trees are extremely low, and have branches very nearly horizontal. Here H has nearly the minimum value. Only very few trees are higher, and have branches inclined to the axis of abscissæ, and the improbability of such a tree increases enormously with its height. The difficulty consists only in imagining all these branches infinitely short.

Finally there is the difference between the ordinary cases, where H decreases or is near to its minimum value, and the very rare cases, where H is far from the minimum value and still increasing. In the last cases, H will reach, probably in a very short time, a maximum value. Then it will decrease from that value to the well-known minimum value.

Paris, April 6. LUDWIG BOLTZMANN.

The Recent Auroral Phenomenon.

ON the evening of March 13, from 7.35 to 8.5, Greenwich mean time, I was a spectator of the abnormal display of Aurora Borealis which attracted so much attention at various places throughout the country. It appeared here as a belt of light spanning nearly the whole sky in a great circle from east to west. When first noticed by me at 7.35, the streak extended from near the hind quarters of Leo to the head of Aries, or from R.A. 169°, Decl. + 16° to R.A. 24°, Decl. + 22°.

At the time the streak was altogether cometary in appearance, beginning in a fine point, but it gradually changed in form, moving at the same time towards the south. Eventually it also shortened so considerably that just before my last view of it, it only extended from γ Geminorum to γ Ceti. Its greatest breadth was about 12°.

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The following figures represent the most striking phases, as nearly as may be at intervals of five minutes.

Fig. 1 shows how the streak extended from the cometary head so as to form a long wavy tail, and represents also the streak at its greatest length. As indicated in the sketch, there was a central portion much more brilliant than the rest, running from the head into the body of the streak.

In Fig. 2 the streak is seen when it had more the appearance of a rainbow than of a comet; and it was very noticeable that one side—that towards the north—was much brighter than the other.

Fig. 3 shows how the "head" began to shrivel up—shortening the streak. The glimmering appearance of the "shrivelling" put me very much in mind of the motion of the air over a "hot heap" (of slag); the tail end began to broaden out somewhat.

In Fig. 4 the streak has taken a very pronounced arrow-headed shape, and, as if to complete the resemblance, the shimmering part took the form of the feathering; whereas in the preceding figure it had more the appearance of comb-teeth. The more brilliant parts are indicated by darker shading.

North.

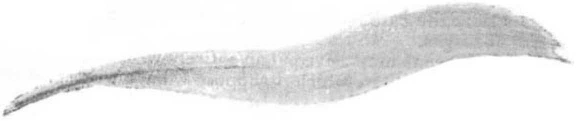

Fig. 1.

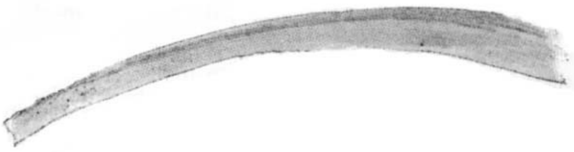

Fig. 2.

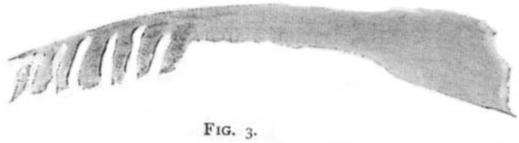

Fig. 3.

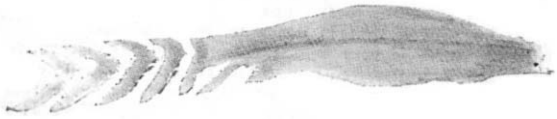

Fig. 4.

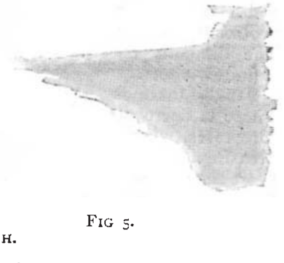

Fig 5.

South.

In Fig. 5 the streak has considerably shortened and broadened out in the west, where it soon afterwards mingled with faint auroral rays which had come round from the northern horizon.

I may say, in general, that the appearances were singularly noticeable and brilliant. The sky was very clear at the time, and every star was visible through the most brilliant parts of the streak. During the time the streak was visible there was a faint display of aurora on the northern horizon, which, as I have already said, worked round to the west and caught the last of the streak.

Jas. G. Richmond.

Muirkirk, N.B.

THE AGE OF THE EARTH.[1]

Ill-health has hitherto prevented my making the comments which seemed called for by Lord Kelvin's friendly article of March 7, in reply to my communication of January 3. Perhaps I may be allowed not merely to restrict my remarks to this article, but to deal more generally with the subject, in the hope of clearing away the misapprehensions which exist between modern geologists and palæontologists, who are no longer uniformitarians, and physicists who are represented by Lord Kelvin.

The arguments as to the age of life on the earth are based on considerations of (1) geology and palæontology; (2) tidal retardation and shape of the earth; (3) the cooling of the earth from an initially hot condition; (4) the age of the sun.

(1) From geology. The leading geologists declare that the great thickness of sedimentary rocks created since the Lower Cambrian, which are almost the oldest fossiliferous rocks, can only have been produced during many millions of years.

It is difficult to get geologists to give even wide limits for the age of the Lower Cambrian.[2] Their calculations are based not upon the rate of accumulation of sediment in one of our quiet oceans, but upon the rate of degradation in valleys where the rate is greatest at the present time. They make this declaration, thinking that for the last thirty-three years it has been authoritatively declared by physicists that such an estimate is absurdly great. I have no doubt that they have done their best to keep this estimate as low as possible, for they have a great interest in making geological theory agree with physics. Some physicists tell them that the flaw in the geologists' reasoning consists in their not taking into account the much greater tidal actions of the past. When tides rose and fell many hundreds of feet, and swept over tens or hundreds of miles of foreshore, there must undoubtedly have been a more rapid formation of sedimentary rock than anything of which we now have experience. The geologists' answer is:—We acknowledge that all nature's actions were on the whole, possibly, more intense in the past. We know from Prof. Darwin's development of Prof. Purser's theory that the moon was undoubtedly nearer the earth in palæozoic times, and the tide influence was therefore greater. But there seems to be no method of even approximately calculating how much greater the tidal influence was. Whilst one great astronomical authority speaks of tides of 500 feet deep in palæozoic times, Prof. Darwin himself thinks that two or three times as great as at present may be an excessive estimate. There is a good deal of geological evidence for much smaller seas than at present, and even if tidal influence were greater the actual tides may have been much smaller than now. Of positive evidence in our favour, we have the fact that numerous examples exist of palæozoic rocks which are identical in almost every physical way with tertiary rocks, and it is difficult to believe that they can have been deposited under very different conditions. Again, nearly all the old sedimentary rocks were laid down near coasts where tidal action would be most violent. Yet even low down in the Cambrian we find the remains of creatures

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[1] In this paper free use has been made of many suggestions from Prof. Fitzgerald.

[2] Their data are of this nature:—Of fossiliferous rocks successively formed the total thickness may be taken as not less than 80,000 feet. Over the areas of the basin drained by many rivers the rate of denudation is known with sufficient accuracy for approximate calculation. Of the basin of the Mississippi a thickness of one foot of rock is removed in 6000 years; the Ganges, 2358; the Hoang Ho, 1464; the Rhone, 1528; the Danube, 6846; the Po, 729; the Nith, 4723 (Sir A. Geikie, Geol. Soc. of Glasgow, 1868). I have heard that Prof. Sollas demands less time than other geologists; but since this paper was written, I have seen (Nature, April 4) that even he does not care to put the age of the Lower Cambrian at much less than 17 million years.

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